

Prof. Chase exhibited diagrams representing certain mathematical and astronomical relationships of length, orbital movement and planetary distance, which he stated and described, including in his subject matter of discussion the possible influence of the meteor-belts.

Pending nominations Nos. 792, 793, and new nominations Nos. 794 to 802 were read.

On motion, the Committee appointed at a previous meeting to consider the expediency of an exhibition of the Progress of Science in the last hundred years, was discharged from further consideration of the subject.

A request by letter from Mr. Etting that the Society permit the exhibition of its original draft of the Declaration of Independence by the city in the Museum of the City Hall, was, on motion, referred to the Curators to report.

And the meeting was adjourned.

ON SOME DISPUTED POINTS IN PHYSIOLOGICAL OPTICS.

BY HENRY HARTSHORNE.

(Read before the American Philosophical Society, April 21, 1876.)

I. ON THE THEORY OF ERECT VISION, WITH INVERTED RETINAL IMAGES.

As it has been ascertained, by both mathematical and physical demonstration, that the image of every object seen must be inverted upon each retina, several explanations have been offered for the correspondence of our sight with the actual position of visible things. The most prominent views advanced are the following:—1. That we do see everything inverted, but that the correction has been obtained, and has become habitual and momentary, through *experience*; 2, that the reversal of all images is effected by the crossing of the filaments of the *optic nerves*; so that, e. g., all the filaments from the upper part of the retina go to the lower part of the optic ganglia at the base of the brain, and *vice-versa*; 3, that we do not mentally regard the image or picture *upon the retina* at all, but look *from the retina, at the object*; or, as one authority upon the subject prefers to express it, “the local change excited in the retina must be conveyed to the optic nerve, communicated to the brain, and again, in an inverted direction, projected outward; through this double inversion the projected image corresponds to

the object, and we therefore say we see the object when only the projected retinal image is before our eyes.”

Against the hypothesis of the correction of inverse vision by *experience*, we have, first, the clear and, so to speak, imperative testimony of every one's consciousness; secondly, the very satisfactory observations and experiments of Spalding and others, upon newly hatched chickens and new-born pigs. The chicken just out of its shell, or one, after hatching, hooded for a day or two and then allowed to see, will *at once* locate an object brought near it (as an edible seed or grain), seizing it accurately with its bill; and will, also, *at once* run in answer to the cluck of the hen, almost always in a *direct line*. Similar facts, precisely, have been observed with pigs immediately after their birth. Thus, *in these animals*, the non-necessity of experience, even for the visual measurement of distances at short range, is proved; much more then must it be impossible that experience should be needed to set things upright, they being inverted according to actual vision. While analogy here only affords a probability as to what is true with regard to human sight, that probability is nevertheless very strong indeed: not that correct visual impressions in all respects are *congenital* with man, as our observation of infants does not seem to show; but that at least the simpler elements of vision attend in their development the maturity of the eye as an organ; and that among these elements, the sight of objects as not inverted must be one of the simplest. If it be said that the readiness for use of the senses and faculties of some new-born animals is a result of hereditary transmission, a *race-experience*, accumulated through long periods of time, I have nothing to say against this as, in itself, a not improbable hypothesis, in regard to the mode of *origin* of such endowments. But the idea of experience which is involved in the view just above-mentioned, is quite different from this last. Experimentally acquired corrections of positive sensory perceptions never go so far as to *annul* the perception which has to be corrected, to such an extent that the *process* of correction *cannot be ascertained by consciousness*.

On the hypothesis that the inversion of the retinal images is effected by a uniform decussation of the optic nerve-filaments, it is needful to remark only three things: 1, that this must be a pure assumption, no such mode or plan of arrangement of the filaments of the optic nerves being anatomically discoverable; indeed, a much more general and intricate plan of exchange of filaments, between eye and eye, optic ganglion and its fellow, and all the parts of the two eyes and the two ganglia, having been proved to exist; 2, that such an explanation is opposed to all the analogies of nerve-distribution, in regard to the localization of impressions; the relative *position* of the nerve-trunk or its filaments, never affecting much if at all, our perception of the locality of any sensation; all notions of locality or direction, under normal circumstances, being obtained by means of, and *at the organ*, not the transmitting or communicating nerve, of sense which is concerned; 3, that the same reversal of position of the image or picture on the retina produced by rays from an object seen, must occur *horizontally* as well

as vertically, the rays from the right side of the object crossing those from the left side as they enter the eye, so that (as in a mirror) what is right side in the object is left side in the retinal picture, and conversely; the difficulty of the anatomical explanation referred to being at least doubled by this *complication* of reversal.

It remains here for me to assert my adherence to the opinion, which is generally growing in favor with physiologists, that much the nearest approach to a solution of the problem of upright vision with inverted retinal images must be expressed in terms essentially conforming to the 3d of the hypotheses above briefly stated. The phraseology used in the passage which I before quoted, making "an image projected outward" that which (not the object) is seen, does not appear to me to be necessary, although it has convenience in describing the analysis of some of the phenomena of vision.

That which it is especially my design to remark upon in this connection is, the clear illustration furnished by our visual experience, and sustained by that of our other senses, of the *externality* belonging to, and inherent in, all sense-perceptions. *Direction* of sound is apprehended, even with only one ear open to receive it; the precise *rationale* of our recognition of the direction of sounds not yet being agreed upon amongst physiologists. I believe that it is obtained by the exquisite sensibility of the orifice, and parts near it, of the external meatus of the ear; a sensibility intermediate between auditory and tactile sense; a kind of gradation existing here, which, there is reason to think, has many illustrations in the partially differentiated sense-organs of lower animals. So, also, we judge, in case of touch, of the *direction* from which anything comes, a ball, for instance, striking the hand, by reversing, as it were, the central axis of predominance of the impressions made; which is analogous to the ocular visual axes, whose correspondence gives us single object-perception in sight.

II. ON EXTUITION AS A NEW TERM IN PSYCHOLOGY.

My reason for dwelling at some length upon the above points of statement is, that I conceive it to be of consequence in psychology (which in regard to sense-perception, especially, is inseparable from physiology), that a more distinct apprehension should exist than has hitherto prevailed, as to the essential, original and inherent, *externality* of the report made to our consciousness by *all* our organs of sense, of the impressions made upon them. In the but partially inductive study of psychology of past times, to which the term "metaphysics" may be, without disrespect, applied, the idea of the Cartesian formula, "Cogito, ergo sum," has been accepted in too exclusive or monopolizing a manner. This was, and is, a deduction occurring in the self-consciousness of the mature philosopher. Could a child, a year old, give its mental experience, so as to define its way of arriving at the knowledge of the *ego*, of its own personal existence and identity, it would doubtless say, "*Percipio, ergo sum.*"

Not uncommonly, authors on psychology assert, in effect, and sometimes

in their very words, that sensation is in the first place always *intuitive*; the process of perception then following, by which a reference of the quasi-subjective impression of consciousness is made, to an external cause. Now I believe that we are justified in denying the correctness of such language; and refusing to admit that sensation is *per se* intuitive, in any proper sense of that word. *Subjective* it may be, just in so far as it is an affection of the subject whose organs of sense (and through them the consciousness) are impressed. But I would insist that the affection of consciousness in sense-perception, nay, in primary *sensation*, is distinctly *objective* in its nature; at least to the extent that the bodily organ affected (not the central *ego* of the metaphysician, nor the brain which receives the terminations of the nerves of sense) is always, to us, the *immediate seat* of sensory cognition; while, at least in the cases of sight and hearing, *externality* belongs, by the law of our constitution, to sense-perception, in its primary nature; not needing a secondary process to attach the inference of outwardness to it. We cannot explain this farther than to say that it is the fact or law of sense-cognition, according to our bodily and mental constitution; but this is true of any and every other view of the subject which may be taken, whether old or new; and the legitimate aim of psychological science must be, as with any other science, to find by observation, experiment and careful induction, *what are* the facts and laws of mind, in all its actual relations. I believe that it may promote definiteness of thought upon this subject, to introduce a new term into psychology; namely, *extuition*. While the word intuition is, as familiarly used, well adapted to denote the reflective process, by which *ideas of reason* are obtained, it appears to be altogether misapplied when the same word is made (as by some leading authors it is) to indicate also the mental affection or act occurring in *sensation*. The importance, in my judgment, of this addition to the language of philosophy, of so explicit a term as *extuition*, conveying a meaning for which there is now actually no existing word, and scarcely a simple English phrase, is the *raison d'être* of the present communication. Word-making has always against it a strong presumption of uselessness, if not of impropriety; but in this case, the presumption seems to me to be over-ruled by a real necessity of thought and of expression. By means of the thought which is intended to be thus expressed, a satisfactory antithesis, and (as it is *inductive*) a tenable refutation, of Berkleyan idealism may be obtained; through the aid (too often overlooked by psychologists) of some of the most clearly demonstrable facts of physiology. Our sensorial consciousness affirms the reality and externality of the objective world, no less *simply, directly and positively*, than our reflective consciousness affirms our subjective being.

III. ON OCULAR COLOR-SPECTRA AND THEIR CAUSATION.

In order to introduce a few observations which, if not novel, have at all events been but seldom noticed, and to bring forward what I believe to be a new explanation of a remarkable group of optical phenomena, it is need-

ful to refer to some facts familiar to most of those who have given attention to physiological optics.

When the eyes have been directed for a short time to a *bright* object, as a piece of polished silver, and they are then turned towards a white wall or other light surface, a *dark* spot or figure, having the outline of the bright object first seen, is beheld upon the white or light surface. Or if, on the contrary, a *dark* body, as an inkstand, for instance, be looked upon steadily for awhile, on turning the eyes toward a light ground, a brilliant *white* corresponding figure is seen. These are called *negative spectra*. If, now, instead, we look fixedly at a *colored* object, and afterwards turn the eyes to a white ground, or, if the colored body be *upon a white surface* and, after a time, this body be *removed*, we will see, in either case, a spectrum having the *color complementary* to that of the object beheld. These are, also, commonly called *negative spectra*; the expression *complementary color-spectra* will designate them more accurately, without regard to theory.

It has been noticed that, near sunset, the rays of the sun passing through an orange-colored cloud cast bluish shadows; and likewise, the shadows of objects seen behind red curtains are apt to be green. If the light either of the sun or of a strong artificial light be made to pass through a pane of colored glass, so as to fall upon a white ground, and a slender object, as a rod, or the hand, intercepts the colored light, the shadow thrown has the *complementary* color to that of the transmitting pane. Red glass will thus throw green shadows, green glass red, orange glass blue, &c. If we look at the shadow, so thrown, through a tube, so that it alone is seen, it is perceived as a shadow without color. Also, if the same shadow falls upon a *black* surface, no shadow appears.

The above mentioned facts have all been repeatedly observed; and but one explanation, so far as I can ascertain, has, as yet, been proposed for them.* It is that suggested by Dr. Thomas Young, and accepted by Helmholtz. Young's theory of colors, being the application to vision of Johannes Müller's general theory of special sensation (as depending upon the character of the sensory *organ* more than on that of the external cause), asserted the existence of different susceptibilities to color rays in different portions of the retina; or among the different optic nerve-filaments. With our present knowledge of the minute structure and relative functions of these parts, such special susceptibilities, viz: to red, green and violet light, as Young designated the primary colors, or to red, yellow and blue, as they are more commonly named, may be referred to the posterior layer of the retina; the layer of *rods and cones* of Jacob and Max Schultze. Helmholtz adopts this theory of Young, as the only one giving any clue at all toward solving the

*Since this was written, I have read, in Lardner's treatise on Optics, a brief statement of Plateau's theory, of an "oscillatory movement" of the whole retina, when affected with light, in connection with what he designates as a resistance of the retina to the action of any color, and a tendency to resume its ordinary condition, with a more or less intense force. There seems to be extremely little evidence in favor of this theory; and it has apparently fallen out of view in later works.

problem of the vision of colors; which, as the great German physicist declares, had baffled not only the powerful mind of the poet-philosopher Gœthe, but many able students of physics and physiology besides.

There seems to be no good reason for hesitating to accept this theory, of the special responsiveness of certain retinal nerve-elements to particular luminous rays. Helmholtz supports it by at least one striking fact in the comparative histology of the eye; viz: that in a number of birds and reptiles, many of the rods of the retina contain, at their ends which are turned towards the light, minute drops of colored oil; in some red, in others yellow; while others of the rods in the same eyes are entirely colorless. Also, he refers to the analogy of the *organs of Corti* in the cochlea of the ear; which are supposed, with considerable reason, to vibrate in accordance with particular notes of sound; and, also, to some remarkable external filamentous appendages to the organ of hearing in certain marine crustaceans; which Hensen, of Kiel, has shown to be set in motion, some by one, and some by other kinds of sonorous vibration. These last facts recall the admirable and similar series of experiments of Prof. Mayer, upon the vibrations of the antennæ of mosquitoes. I believe that there is much importance in this general theory of undulatory consonance, and of special responsiveness, of the minute elements of the organs of sense. It is in the *manner* of the *application* of this theory by Helmholtz, and, with him, by all other late physiological writers known to me, to the explanation of *negative* and *complementary color spectra*, and *color shadows*, that it appears to me a deficiency exists, fatal to the reception of that current explanation; and suggesting, if not proving, an almost or quite contrary view of the same facts.

The application I here mean to object to, is this: that negative and complementary color spectra and color shadows are all explained by partial or local *fatigue* of the retina, under impressions of light, so that the part of the retina impressed, e. g., by a particular color, becomes, through fatigue, less sensitive to the same color, kind, or degree of light; and therefore an impression is, during the time of that fatigue, made upon our visual consciousness only by the opposite or complementary rays; these affecting those parts or elements of the retina which are fresh, not having been wearied by use. According to this, when we look for a time upon a red object, those rods and cones, or nerve-cells, or nerve-filaments (or, as Prof. Draper would have it, portions of the *choroid coat behind* the retina) which are affected by red rays, become exhausted; and so, when we turn toward a white ground, we are, for the time, red-blind; while the *green-seeing* capacity, so to speak, is fresh and vigorous. Thus we see, at that time, only green; or, the complementary color in any other like case. This *fatigue* theory is what I am constrained, notwithstanding the very eminent authorities by whom it has been hitherto supported, to call definitely in question. Let me here mention the observations which first led me to look for a different view of the whole subject of these kindred phenomena.

If a piece of thin white paper, such as is commonly used, on account of

its light weight, for foreign correspondence, be laid over a piece of brightly colored material (paper, silk, flannel, or any other) and then on the latter a small black or dull colored strip or fragment of any kind be laid, under the paper, this *fragment or strip* will appear to be of the color *complementary* to that of the *larger and brighter ground* upon which it is placed. Any color will answer for this experiment; although the effects are most distinct with red and green grounds; tolerably so with bright yellow; least so with blue, purple, and all dark shades. It makes no difference what is the *hue* of the *fragment or strip* upon the brighter ground under the paper, so long as it is duller or darker, as well as considerably smaller, than the ground on which it is laid; it will always have the color complementary to that ground. Put, for example, together, under the thin paper, on a bright green ground, strips of blue, red and yellow glass, and one also of black worsted or paper; all of these, seen through the paper, will be, *alike*, distinctly *red*.

Now, on observing these facts, let it be noticed, especially, that the effect produced, the complementary color given, is *instantaneous*. The moment you look at the bright ground with the dull or indifferent strip over it, you *at once* see that strip with the *complementary* color. Lift the paper, and the real hue, or blackness, is seen; replace it, and it becomes again changed as before. There is here *no time for fatigue* of the retina, or of any part of it. Again, if the bright ground have, around it, a *margin* of the paper which is laid over it, that margin will have a faint but distinct *flush* of the complementary color. If the bright ground be *moved* to and fro, this over-flush of color may be, sometimes beautifully, shown, following the line or edge of the moving mass of strong color; and most distinct near the edge. This effect, also, is instantaneous; there is no opportunity for *fatigue* in any way to explain it.

Finding then, this "fatigue" theory quite insufficient for these facts, attention to the phenomena of *color-shadows* above referred to, has impressed me with a belief that the same theory falls short with them also. They are, likewise, immediate, momentary effects. You look at the white ground, bathed in colored light, which light is partly intercepted by a narrow opaque object; and *at once* you see the shadow of the latter, having the complementary color; there is no possible time for fatigue, or for loss of any portion of the retinal sensibility.

Consideration may next be in place, upon a remark of Helmholtz,* which, though only parenthetically made by him, has very considerable importance. Bright objects, as flames, or the sun, when looked at, give subsequent spectral effects unlike those described as negative. They are, indeed, for a time at least, *positive* spectra. Helmholtz speaks, moreover, of from *half a minute to five minutes'* contemplation of a bright or colored object to produce the *ordinary* negative or complementary color spectrum by looking upon a white ground. I have found from *five to ten seconds* ample, with a kerosene students' lamp; and less time still, with strong sunlight.

*Popular Lectures, transl., 1873.

My experiments with sun-spectra, designed for the purpose of examining more particularly the above facts parenthetically mentioned by Helmholtz, were as follows: At about three o'clock on the afternoon of a clear day I held between my eyes and the sun, a pane of colored glass; using, at intervals, successively, four colors; blue, green, red and orange. After a few seconds, I turned my eyes from the glass upon a white-washed wall. In each case, a strong complementary (so-called negative) color spectrum was seen upon the wall; but, on *closing* my eyes, an almost equally intense *positive* spectrum, having, that is, the *same* color as the stained glass just looked through, appeared. Opening my eyes, the *complementary* spectrum returned; again closing them, the *positive* one; and so on, for half a dozen or a dozen times in succession. A member of my family, and, on another occasion, Prof. E. J. Houston, repeated this observation, with the same results; and I also did so, with entire success, with a magnesium light, at night. These experiments seem to me quite fatal to the supposition that retinal fatigue can account for any class of spectra such as have been above considered; for, if ordinary luminous impressions produce temporary fatigue and loss of sensibility, *stronger* impressions ought to produce still *greater* fatigue and greater loss of sensibility, or partial color-blindness, from that cause; whereas the reverse is the fact. As Helmholtz states, and as Newton found in his famous experiment, which proved dangerous to his sight, very bright objects, such as the sun itself, give *positive* spectra. In Newton's case, after gazing directly at the sun, its image did not pass away from his vision, whichever way he looked, for two or three days. Looking through colored glass, my daughter obtained a solar spectrum which continued for nearly twenty-four hours. In my experiments just mentioned, if it were possible for fatigue to account for the *negative* or complementary spectrum seen with open eyes, what conceivable relation (I certainly think none) can such a cause have, to the *positive* spectrum seen when the eyes were *closed*? If fatigue might *take away*, so to speak, the capacity to see green light when the eyes were *open*, it is against every "law of parsimony" in science to suppose that the same cause could *confer* the capacity to see a green image or spectrum, when the eyes were *shut*.

Another experiment, decisive against the fatigue theory, is one by which a *derived* or *secondary* spectrum may be obtained, as follows: a small square or circle of white paper or muslin is placed in the middle of a large brightly colored ground, red, for instance; and is steadily looked at for from a quarter to half a minute. The white central spot will acquire a tint of the complementary color (green, upon red), increasing in depth with prolonged attention, and especially strong at the nearest point of distinct vision. Then let the eyes be turned to a white ground; there will appear upon it the usual complementary (in the case supposed, green) spectrum, of the ground; but, at the *centre*, there will be also a clear *positive* (red) spectrum, corresponding in place and size, with the *white* central patch. Here, of course, that part of the retina which had received *only white* light, could not, on the fatigue hypothesis, be supposed to lose its sensibility to green,

through exhaustion from special use; while, on the theory which I am about to set forth, the mode of causation of the secondary as well as of the primary spectrum appears quite clear.

It remains, then, to attempt a definite answer to the question, what explanation, conforming with all these facts, can be substituted for that which I have ventured to pronounce unsatisfactory. I make an endeavor in this direction with diffidence; not possessing so precise an acquaintance with physics as might enable me to deal as an expert with so difficult a subject.

Allusion has been made above to four kinds of phenomena; which may be thus briefly named, *en résumé*:

a. Those of ordinary *color spectra*, primary and secondary, seen with moderate light.

b. *Color shadows* thrown under transmitted light.

c. *Over-tints* and *complementary shades*, seen through thin paper, on a brightly colored ground.

d. *Solar color spectra*, positive and complementary by turns, according to whether the eyes are closed, or opened towards a white ground.

These all appear to me to have a common character, and to require an essentially identical explanation.

Take, first, the ordinary color spectra, such as may be obtained with good lamplight. Looking at a red object for a few moments, one then turns the eyes to a white surface; a green spectrum is seen. Why is this? Because, to use the simplest and least hypothetical phraseology, the eyes are *charged*, *saturated* with red light; and this, having a certain strength, is *neutralized* by the red rays in light reflected from the *white* surface, so that only the *remaining*, complementary, green rays of that light affect the sight. Translating these expressions into language in accordance with the undulatory theory, I would say that, when a brightly colored object is looked at, those rods and cones, or minute retinal nerve-elements, which respond in vibration to the luminous ether-waves of the color reflected to the eyes, are excited to motion thereby; and by "irradiation," or communication of vibrations, all retinal elements which have *the same period of vibration* are made to partake, in some degree, of this movement. Then when, turning from the colored object, *white* light, consisting of all the color-rays or waves together, impinges upon the eyes, those ether-waves of the white light which belong to the color first acting on the retinal nerve-elements, *interfere with*, and for the time relatively diminish or *annul* the special vibrations already produced in the retina; leaving the *other* waves of white light to take effect upon the retinal elements which respond to or "resonate" with them, so that the *complementary* color only is seen.

Very probably *relative diminution*, rather than total arrest, of the special retinal vibrations, is what occurs. All our perceptions of light and color are, to a considerable extent, dependent on the *relative intensity* of light from different sources or of different kinds. When, then, in a beam of white light, a *portion* of certain color rays is, in effect, arrested by previously existing retinal vibrations of the same period, although the *remaining* rays

of the same color might affect the sight *if they were alone*, they are, as it were, overpowered by the unimpeded complementary color-rays. This consideration appears to me to meet an objection to my "interference" theory, suggested by Prof. Houston; derived from the necessary relation of *wave-length* of rays for interference. If only a moiety of the ether-waves of a certain color in a beam of light afford the required opposition of phase in undulation, their arrest or subtraction may yet explain the *relative* lessening of the intensity of that color, so as to cause it to cease to be perceived; the complementary color-rays having twice as great a strength.

When color shadows are produced by intercepting transmitted colored light, the shadows must fall *on a white ground*, or they do not have the color complementary to that which is transmitted. Here, again, the eyes becoming saturated with a certain color, *its* specially responsive nerve-elements in each retina are set in vibration; and, as in the other case, interference occurs between these vibrations and the ether-undulations corresponding with the same color, in the reflected (diffused) white light. Let the shadow of the intercepting object fall upon a *black* surface, and the complementary color disappears. Nor will it be seen when only *monochromatic* light is present in the apartment.

Again, the above-described *over-tints* or flushes, and complementary interrupting spots, strips or bands, upon a colored ground seen through thin white paper, depend upon the *white light reflected* by that paper, for their complementary color. If the paper be removed, the actual difference between the ground and the spots, strips or bands laid upon it, will be clearly seen. Replace it, and *at once* a strong contrast re-appears; as before, I hold, resulting from the interference of equivalent vibrations.

Lastly, when a very strong impression is made upon the eyes, as by looking at the sun through a colored glass, the excitement of the nerve-elements is so great as to *persist* in the same manner; producing continued sensation of the same color, when the eyes are closed, for some time. But, when the eyes, in this state, are opened upon a white ground, again the waves of the *same* color-rays in the white light *kill*, as it were, that color in the eyes; or, in other words, arrest, for the time, enough of the existing retinal vibrations to annul their effect in sensation, so that only the remaining (complementary) rays of light are perceived. Bright reflected white light thrown even upon the *closed* eyes will have this effect.

An analogous explanation will suffice for the *secondary* or *derived* spectra, mentioned on a previous page. Looking steadily at a small white spot on a red ground, for example, the white spot becomes green; turning thence to a white ground, a large green spectrum is seen, with a red spot in the centre. The latter I call a secondary spectrum. While looking at the *red ground*, vibration of the *red-resonating* retinal nerve-elements began, and was extended, by irradiation, to those covering, in vision, the *white spot* in the centre; being, there, sufficient to *neutralize* the *red* rays of the white, and thus allowing only green to be seen, by vibrations of the *green-resonating* rods and cones of *that part* of the retina.

On turning then to a *white ground*, most of the retina being *charged with red* by the previously contemplated red ground, the usual *complementary, green*, spectrum of that ground is seen; but at the *central* part of the retina, whose *green-resonating* elements have been set in vibration (as above said), *their* vibrations are annulled by the *green rays* of the white ground; leaving only the red rays of that ground to take effect, and producing a (secondary) red spectral spot upon the green.

These facts are, it appears to me, all closely similar in nature to the commonly observed interference occurring between diffused sunlight and lamp or candle flame; and, also, they have a definite relation to the mode of production of the Fraunhofer dark lines or "absorption bands" of the spectroscope; while they suggest some further speculations in regard to what may be the molecular conditions of reflecting surfaces which give to our vision impressions of different colors, to enter upon which, at present, would extend this communication to too great a length. I may simply add that, on careful examination of ocular spectra obtained by looking at colored objects by lamplight, as well as by sunlight, I have become convinced that the influence of light entering through the *closed lids*, if they are uncovered and within range of a luminous source, such, even, as that of a student's lamp, is not at all indifferent. A spectrum, obtained as usual, seen with closed eyes, and fading away while they are covered, may be *renewed* by approximating the eyes, *still closed*, to the light; and this again and again, several times. In order to test yet farther the sensitiveness of the eye, when closed, to light, I placed several pairs of glass slips, of different colors (blue, red, green and orange), in the hands of an assistant; laying them, then, as given to me, in turn, upon my closed lids, while my eyes were turned towards the afternoon sun. In every case, I could discern clearly the color of the glass; but only in the direct rays of the sun; diffused daylight, and lamplight failed, with me, to give more than a doubtful discrimination of color, even with near contact of bright translucent objects, while the eyes were closed.

Incidentally, I may mention also, my careful repetition of a very curious experiment mentioned by Sir. David Brewster in his Optics, and brought to my notice by Prof. E. J. Houston. A strip of white paper is held vertically, about a foot from the eyes; the attention is then fixed upon an object at a greater distance, so that the slip of paper is seen double; and at the same time a lighted candle is brought near to the side of one eye, so that its light will affect that eye strongly. The image of the strip seen by that eye (if the *right eye* be illuminated, the *left-hand image*, and *vice versa*) will be *green*, and the other *pale red*. I find that, when these two images have been obtained, one eye can be closed, but the same color still remains. Brewster asserts that when *two* equal candle-lights are used, one being held near each eye, two *white* images will be obtained. With me, this does not prove to be the case. I see, with two candles, *two green* images instead; and, fixing the eyes on the strip for *single vision*, with two candles placed as before, it is seen of a *distinct green* color. Without insisting upon it, I would pro-

pose as a hypothetical explanation of these results, the throwing of *red light* upon the retina by the light of the candle passing *through the blood vessels* of the eye-ball; this red light, in the eye most illuminated, *killing* the red-light vibrations of the retinal elements, so that only green is seen. The pale red of the image seen by the *unilluminated* eye is, with me, of doubtful distinctness.

If the facts and arguments which I have set forth in this paper impress other minds as they have done my own, especially in view of the *instantaneousness* of many of the phenomena described, and on which I have wished to lay particular emphasis, it may, at least, be concluded, that the commonly accepted theory, proposing to account for negative and complementary spectra by partial *fatigue* and diminished *sensibility* in the retina, will not suffice; and ought to be abandoned, as not at all reconcilable with several clearly demonstrable facts. Should my above attempted explanation, by *interference* (or *saturation* of the retina with certain luminous rays, and *neutralization* of a portion of the same by equivalent rays present in reflected white light), not prove altogether satisfactory, it may be an interesting task, not without importance in physiological science, for some one well-versed in the physics of the subject to give it such farther attention as may solve the problem, in a manner which meets, as has certainly not hitherto been done, all of its conditions. I ask leave to verify some of my assertions as to the facts referred to in this paper, by repeating here a few of what appear to me the most decisive of the experiments which have been mentioned.

Stated Meeting, April 7, 1876.

Present, 19 members.

DR. LECONTE, in the Chair.

Letters of acknowledgment were received from the Batavian Society at Rotterdam (XV ii, 94); the Royal Observatory at Bruxelles (XV ii, 94); the Royal Astronomical Society (XIV); the Society of Antiquaries (95), and the Victoria Institute in London (95).

Letters of envoy were received from the Zoologico-Botanical Society at Vienna, March, 1876, and the Meteorological Office at London, dated March 20, 1876.

A letter requesting missing Nos. XIV, ii, 62, 88 was received from the R. Astronomical Society, dated February 16, 1876.